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unprecedented. The Darling River, in 1839, was merely a chain of water-holes; and again, ten years later, it was but little better; in 1851 the river was so dry that grass had grown in it, and in fact it was the only feeding-ground available; in 1863 and 1865, and again in 1868, the water was very low. In 1870 the great wet season began, and it was this superabundance of rain which led to the overstocking of the country and the consequent disaster. It is clear that those who occupy the western part of the colony have to encounter some very bad seasons, intermixed with some very good ones; and arrangements should be made by which the stock which in wet years may be supported, may be transferred to more favorable regions when the grazing fails, or to *abattoirs*, where it can be killed, and turned into canned or frozen meat. There now seems to be some hope for a return of rain, as the natives are reported to be moving to higher ground, and the white ants are said to have commenced building their curious elevated dwellings, which serve them as places of refuge during wet weather. These two indications are referred to by Australian journals as unfailing evidences of a probable change in the weather.

PERHAPS IN NO OTHER branch of zoölogy has the instability of nomenclature become more burdensome than in ornithology. He who, after a lapse of even a few years, attempts to renew his acquaintance with our bird fauna, is depressed and disheartened by the innumerable strange names and tedious lists of synomyms that he everywhere encounters. The Ornithologists' union has recently published a new check-list of North American birds that calls attention forcibly to this evil, but which also contains an excellent code of the principles and canons of zoölogical nomenclature, that, it is hoped, will be of some avail in lessening it. The committee appointed to draught this code was composed of five of our best students of vertebrate zoölogy, and may thus fairly represent the views held by the great body of zoölogists. The most important of the principles therein laid down are: the strict and rigid enforcement of the *lex prioritatis*, without any 'statute limitations' whatever of time; that a 'synonyme once is a synonyme always,' and that the same name cannot be retained for more than one genus in the animal kingdom; that a generic or subgeneric name may be based upon a designated recognizably described species;

and that the original orthography of a name is to be rigidly preserved, unless a typographical error is evident. With most of these principles zoölogists in general will agree. The necessity of inflexibility in the law of priority has steadily become more and more apparent; there is no mean position that does not admit of all manner of abuses, and the same may be said of the use of names that have once been synomyms. The last-mentioned principle is also a very important one. In entomology at least, and especially among many German purists, infractions of this safe rule have become in many cases almost unendurable. Those who, in their zeal for philological rules, amend, alter, or even reject names altogether, forget that nomenclature is not the end, but the means, of science. The Greek might write *aiuopphayia*, but the modern zoölogical classicist would insist upon haematorrhagia. The principle, however, that virtually admits catalogue generic names to recognition, will, we believe, receive vigorous protest from many zoölogists, as subversive of the essential rule that a species or genus must be described in order to be accepted. A specific description does not necessarily contain higher characters, and such characters must be given before a generic name can obtain currency. Students in distant parts of the world cannot depend upon specimens. A tyro can say such and such a species belongs to another genus, and give it a name, but it requires scientific discrimination to point out reasons. As well give to the bird-specimen No. 999 in the national museum a specific name, and leave the student to find out the characters as best he can. Ornithologists sometimes forget that rules applicable to their much-studied class may be intolerable in less-known groups.

PASTEUR AND HYDROPHOBIA.

THE place Mr. Pasteur now occupies in the minds of the world affords a striking example of the extremes to which the popular judgment is liable. On the one hand, we have in the 'Pasteur institute' an organization which proposes to put the new method of curing hydrophobia into operation on the largest scale in all civilized countries. At the other extreme we hear from many points the cry that all of Pasteur's pretensions are fraudulent. These extreme views are equally unwarrantable, and equally illustrative of the lack of sober judgment with which the world receives

such attempts as those of the eminent chemist and philanthropist. The sober-minded man should encourage every form of research designed to promote the interests of humanity; but he should at the same time reserve his judgment until sufficient data are at hand for reaching a well-grounded conclusion.

The efficacy of any method of treating hydrophobia must be extremely difficult to test in a way which shall be at all conclusive. The first difficulty we meet in reaching a conclusion arises from the extreme rarity of the disease. The number of readers of these lines who have ever had personal knowledge of a case of hydrophobia is probably very small. In the returns of the last census eighty deaths are reported from this cause in the United States. But we should regard this number as an extreme limit rather than as a well-established quantity, owing to the possibility of other forms of disease being mistaken for hydrophobia. On the other hand, the number of persons who are actually bitten by dogs which, for aught they know, might have been rabid, is very great. It is certainly to be estimated by thousands, and perhaps by tens of thousands. It becomes apparently much greater when, as during the past year, the public mind is excited on the subject. In such a case it is difficult to ascertain, to the entire satisfaction of the injured person, that any dog which may have bitten him was not rabid. The result is, that it is rarely possible to select any injured person as probably being inoculated with rabies. Of the persons brought into an institute for treatment, it may be assumed that only a small percentage would, under any circumstances, develop the actual disease.

Pasteur's supposed success cannot, therefore, be established as a fact until we have more complete evidence of the circumstances attending the injuries, and especially of the rabid character of the animals which have bitten his patients. Even of the well-established cases of bites by rabid dogs, only a minority ever develop into actual rabies, and this minority may require many months for the graver symptoms to appear. The first certain conclusion must therefore be founded on statistics in which the evidence that the animal was rabid shall be conclusive, and in which every result shall be included. A table showing the termination of all cases treated, and of all similar cases not treated, will ultimately be conclusive, and nothing less will serve the purpose. The efficacy of the treatment cannot be disproved

by occasional cases of failure, unless it is shown that these cases approximate in number those in which no fatal symptoms are ever developed. This also must depend upon the results of a statistical investigation.

No doubt, a profound impression has recently been made by the failure of the treatment in the cases of the party of Russians bitten by mad wolves; but this failure only shows that the treatment may fail in such extreme cases as these, which seem to have been unusually severe. It is quite conceivable that a process which would be entirely successful in cases so mild as to require several months for their development would prove useless when the quantity of virus injected was so great as to lead speedily to a fatal termination. It is significant that the first Russian to succumb was bitten by an animal so ferocious that one of its teeth was left deeply embedded in the flesh of its victim.

If the final conclusion should be against the efficacy of inoculation, are we to denounce the propounder of the treatment as a pretender? By no means. He will still be entitled to all the credit which society owes to a man who makes an honest attempt to promote its welfare. The character of the great experimenter is above suspicion; and the knowledge which he acquires, if not useful in one direction, may be useful in another. Let us, then, wish him well, and, if he fails, let us still award him the credit due to the spirit which inspired his efforts.

THE MALARIAL GERM OF LAVERAN.

DURING a recent visit to Rome, the writer had an opportunity to see, for the first time, a most interesting blood-parasite, which was first described several years since by Laveran, a medical officer in the French army. Extended researches made in Algeria had convinced Laveran of the constant presence of this parasite in the blood of persons suffering from malarial fevers, and that it is not found in the blood of healthy persons, or in that of those suffering from other diseases; also that it disappears from the blood under the administration of quinine, which is recognized as having a specific curative effect in diseases of this class.

There are many circumstances connected with the causation of the malarial fevers which make it appear probable that they are due, either directly or indirectly, to a living organism which finds its normal habitat in marshy places, and